Ro. XXVIII. *Account of the Airs extracted from different Kinds of Waters; with Thoughts on the Salubrity of Air at different Places. In a Letter from the Abbé* Fontana, *Director of the Cabinet of Natural Hiſtory belonging to his Royal Highneſs the Grand Duke of* Tuſcany, *to* Joſeph Prieſtley, *LL.D. F. R. S.*

Read April 16, 1779.

I TAKE the liberty of ſending you an account of ſome experiments which I made at Paris in the years 1777 and 1778 on the air extracted from various kinds of waters: ſome of the principal of which I thought proper to tranſcribe, that you might make uſe of them if you think that they are at all uſeful, and likewiſe lay them before the Royal Society.

I have extracted the air from the water of a well by means of common fire. The water was then made to boil in a large mattraſs of tin, which had a long tube of the ſame metal, which being bent into two different directions was with its extremity immerged in a tub of cold water. The mattraſs and its tube were intirely filled with water: the air which came out of it was received into three different veſſels. The air of the firſt veſſel, by being ſhaken in water, was diminiſhed a little; the air of the ſecond was diminiſhed of half its bulk, or rather more; and the air of the third veſſel was diminiſhed exceedingly. The *reſiduums* of air that remained unabſorbed were more or leſs phlogiſticated.

Another time I obtained almoſt entirely fixed air, excepting a little which remained unabſorbed by water, and was partly phlogiſticated.

A third time the air of the water of a well, obtained as above, was made to paſs through mercury into a tube anointed with oil of tartar, and it occaſioned a cryſtallization juſt like that which the pureſt fixed air is uſed to do.

A fourth time I impregnated with this air a quantity of common water, which abſorbed its own bulk of it, and became by theſe means acidulous, exactly like water with the pureſt fixed air. This water turned the tincture of turnſole red, and precipitated the lime in lime water. Another time a light was ſucceſſively extinguiſhed, and a bird died inſtantly in this air.

The water of the river Seine, filtrated through ſand, as it is drunk at Paris, was treated in the ſame manner as the water of the well. The air extracted from that water was half abſorbed by water, when ſhaken in it; the remainder, when treated with the taſte of nitrous air, gave II−4, II+1 (a); when the common atmoſpheric air treated with the ſame nitrous air gave II−4, II+8. It was therefore ſenſibly better than the atmoſpheric air, which, during three years of experiments made at Paris, I have conſtantly found to be inferior to the air of the Seine water, extracted as above.

Having repeated the experiment, I received the air into two different receivers. The firſt of which, by being ſhaken in water, was diminiſhed in the proportion of ten to ſeven; and by the teſt of nitrous air gave II−14, II+1, III+1, when the common air gave II−12, II+6, III+6.

The ſecond quantity of air was diminiſhed in the proportion of three to one; and when examined by the teſt of nitrous air gave II±0, III±0. From whence it may be concluded, that the firſt air was better than the atmoſpheric air; whereas the ſecond was worſe, and mixed with much fixed air.

Being in doubt whether the tin veſſel employed in the experiment above mentioned might not alter the nature of the air, &c. I made uſe of glaſs veſſels. Having therefore filled one of theſe veſſels, having a long neck bent in two directions, with the Seine water, I obtained ſome air which ſeemed not ſenſibly diminiſhed when ſhaken in water. Having introduced one meaſure and 37 parts of this air into the tube uſed to try the diminutions, it gave with the nitrous air I+19, I+48, when the ſame quantity of nitrous and atmoſpheric air gave II+26, II+6: it is therefore certain, that the air extracted from Seine water is purer than common air.

(a) See p. 343. for an explanation of this meaſure.

Another time I extracted, in the ſame manner, and from the ſame water, the air; one meaſure and 24 parts of which being introduced into the tube, &c. and ſhaken, was reduced to one meaſure − 31 parts, that is, one fifth of it was abſorbed. Treated with the nitrous air it gave I−4, when equal meaſures of common and nitrous air gave I±0: it was therefore better than common air.

A third time I extracted the air, in the manner above mentioned, from the water of the river Seine, contained in three mattraſſes: this air was about one twenty-eighth of the bulk of the water, and it gave with the teſt of nitrous air II−14, II−9, III−9; when the common air mixed with nitrous air, as uſual, gave II−14, II+8, III+8. It is therefore clear, that the air extracted from the Seine water, by the action of fire in glaſs veſſels, is much better than common air, or than the air which is extracted from the ſame water when boiled in tin veſſels.

Another time I filled a glaſs retort, which had a long and doubly bent neck, with Seine water. The water weighed about three pounds. The air that came out of it loſt a quarter of its bulk by being ſhaken in water; and afterwards being tried with nitrous air it gave II−16, II−16, III−16, when common and nitrous air gave II−12, II+12.

This experiment being repeated, the air was diminiſhed of one quarter by being ſhaken in water. One meaſure − 16 parts of this air introduced into the meaſuring tube gave II−32, II−2, when common air and the nitrous gave II−28, II+4.

The water d'Arqeuil at Paris is conſidered as very pure. I filled the tin veſſel above mentioned with it, and received the air that came out of it into three veſſels. Being ſhaken in water, the firſt of them was diminiſhed one fifth; the ſecond, three fourths; and the third $\frac{15}{16}$ by the operation in water. A light burned with a flame, more luminous than in common air, in the firſt air after it had been ſhaken in water. This air being tried with the nitrous air gave II−10, II−10, III−10. The ſecond gave II−10, II−17, III−30, when common and nitrous air gave II−2, II+14, III+14. The third air, before it was ſhaken in water, cryſtallized with the oil of tartar like fixed air. An equal bulk of it was abſorbed by water, which by this mixture became acidulous; it precipitated the lime in lime water, extinguiſhed a light ſeveral times, and killed an animal inſtantly. It is therefore partly fixed air, and partly air which is not only better than common air, but likewiſe than that extracted from Seine water, even when this laſt has been boiled in glaſs veſſels.

The experiments being repeated with the ſame water of Arqueil, but in glaſs veſſels, the air obtained, after being ſhaken in water, was much better than that obtained from the ſame matter, when boiled in veſſels of tin.

I have alſo extracted the air from diſtilled water in glaſs veſſels, and having ſhaken one meaſure – 32 parts of it in water, it was reduced to one meaſure – 35 parts. With the teſt of nitrous air it gave I–6, when equal parts of common and nitrous air gave I–2, which ſhewed that it was better than common air.

I extracted the air again in the manner above deſcribed; but it was not ſenſibly diminiſhed when ſhaken in water. Two meaſures – 49 parts of it, with the teſt of nitrous air, gave I–2, I+8, when common air, &c. gave I+1, I+18: It is therefore better than common air.

I extracted the air from a great quantity of diſtilled water in the uſual manner, and found that it did not ſenſibly diminiſh in water. With nitrous air it gave II−14, II−25, II+25, when common and the ſame nitrous air gave II−14, II+10, III+10; conſequently it was dephlogiſticated air, *viz.* purer than the air of the Seine and Arqueil, which are much better than common air.

I had the curioſity to try, whether any difference would ariſe from boiling diſtilled water in a matraſs of tin inſtead of glaſs veſſels; and found, that the firſt air was diminiſhed one tenth by being ſhaken in water, and afterwards with the nitrous air gave II−13, II−16, III−18, when common air gave II−12, II+8; which ſhews that it was dephlogiſticated air, but not ſo good as that extracted from the ſame water when boiled in glaſs veſſels. The ſecond quantity of air was not ſenſibly diminiſhed in water, and with nitrous air gave II−13, II−20, III−30; that is, it was more dephlogiſticated than the firſt.

The air extracted from diſtilled water is to that extracted from the water of the river Seine as 13 to 32 nearly; whence diſtilled water does not give more air than one ſixtieth of its bulk: but as the air extracted from the water of the Seine is half fixed air, it may be concluded, that the quantity of reſpirable air produced by both kinds of waters is nearly the ſame, and that they only differ a little in purity. It is however true, that other experiments have ſhewn me that water in general abſorbs about twice as much of dephlogiſticated as of common air; for which reaſon, I think, that the reſpirable air of Seine water is rather leſs than that of diſtilled water. Accordingly I have found, that Seine water, after it has been boiled for a long time, abſorbs in forty days about one fourteenth of its own bulk of dephlogiſticated air, when in the ſame length of time it does not abſorb more than one twenty-eighth of common air. This ſeems to be an experiment of very great conſequence, and is much worth notice; eſpecially as it diſcovers a new characteriſtic by which dephlogiſticated air may be diſtinguiſhed from common air; and ſhews, that water abſorbs a greater quantity of thoſe kinds of air, which contain a leſs quantity of phlogiſton.

It muſt however be obſerved, that it is impoſſible to determine exactly the quantity of air that is extracted from veſſels filled with water, by means of fire; becauſe a portion of the air is abſorbed by the water of the tub in the act of its coming forth. It will certainly be more exact to receive the air in veſſels immerſed in quickſilver; but then there are many other inconveniencies to encounter.

It may be almoſt ſuperfluous to mention, that the above related experiments are very uſeful in explaining the reaſon why ſome kinds of water have a peculiar ſharp taſte more than others; and eſpecially why ſome of them precipitate the lime in lime water, rendering it a calcareous earth, and change the tincture of turnſole into a red colour, as I have generally experienced with the well waters at Paris. We may alſo explain from hence, why ſome kind of waters can diſſolve iron, and keep it in diſſolution without depoſition; whereas other kinds of water are incapable of doing it, at leaſt do it much leſs than the pureſt diſtilled water. This is ſoon diſcovered by boiling the water, which will then depoſit the iron which before was diſſolved.

It will be ſufficient, for the preſent, to mention, that I have not only extracted from waters the different kinds of air they contained naturally, but have likewiſe made various experiments upon waters deprived of air, which, being expoſed, have again imbibed the atmoſpherical air, as I hinted above. I have determined the quantity and quality of thoſe airs. In general, I may ſay, that diſtilled water, deprived of air, imbibes again an equal quantity of air of the ſame kind as that it had loſt, and that in leſs than fifty days. Other kinds of water do the ſame, but with this difference, *viz.* that the air they abſorb, after being boiled, is better than that they have loſt; and in this particular they come very near to the nature of diſtilled water itſelf.

If the waters deprived of air are expoſed to common air held in receivers in contact with quickſilver, the air which remains unabſorbed is ſo much more phlogiſticated as a leſs quantity of it remains in the receiver. This experiment deſerves conſideration.

By means of pure water, eſpecially diſtilled water, common air may be changed into dephlogiſticated air, that is, into air much more ſalubrious than the beſt common air which we breathe; and this, for what I know, is the only means of meliorating common air: for all the artificial methods (great numbers of which I have tried) have proved either uſeleſs or noxious, but never ſuch as promiſed to be of any great utility to human kind.

Though I have long thought of applying thoſe experiments to ſome uſe for the purpoſes of life, the want of time and a proper apparatus has hitherto hindered my doing any thing; I now begin to be in hopes I ſhall be able to do ſomething. In the mean time I think it of ſome importance to have it known, that water not only poſſeſſes the property of diminiſhing the noxious part of tainted air, but has alſo the power, and that in a very high degree, of dephlogiſticating common air; which muſt certainly be one of the methods by which nature keeps the atmoſphere in a ſtate conſtantly fit to ſupport animal life, it being certain, that the water in various circumſtances muſt loſe either a part or the whole of that air which it hath abſorbed from the atmoſphere.

It may with ſome reaſon be ſuſpected, that in my experiments of extracting the air from water by the action of fire, the air might be conſiderably altered by the vapour of the water itſelf. As this difficulty was of ſome force, I endeavoured to remove it in the following manner. I introduced into a tube, through water, a quantity of common air of known goodneſs, and I cauſed the ſteam of water boiling in a matraſs, from which the air had been previouſly extracted, to paſs through it. The heat of the ſteam ſometimes made the water occupy above five times the ſpace it did when cold; yet the air ſo treated was not at all altered by it, as appeared by the teſt of nitrous air. The event of the experiment, although repeated various times, was conſtantly the ſame.

I muſt obſerve, laſtly, that having once cauſed the air of boiling water to paſs into receivers filled with, and ſtanding in, quickſilver, I found that the air was better than uſual. I have obſerved the ſame thing when I have cauſed the air to go through diſtilled water into receivers filled with it: which obſervation, if the event of the experiment is conſtantly the ſame, induces me to believe, that the air loſes ſome of its good properties by going through water not very pure; or, which ſeems to be rather more probable, that a quantity of air leſs good is, by the action of the vapours and the heat, extricated from that impure water, and is mixed with the air that comes out of the matraſs; from whence this air is debaſed.

I muſt not omit to mention a new character of equal importance with that which diſtinguiſhes the dephlogiſticated from the common air. This new character has been equally unknown, and deſerves the attention of philoſophers, becauſe at the ſame time it diſcovers a new property of the atmoſpherical air, which I ſhould never have ſuſpected if experience had not offered it to me.

I have found, that common air ſhaken in water, inſtead of being diminiſhed is ſenſibly increaſed in its bulk. The increaſed ſpace is in proportion to the time the air is ſhaken in water, and it begins to be ſenſible even from the beginning, that is, after a few ſeconds. This augmentation I have ſometimes brought to be one twelfth of the bulk of the air, and even more: it muſt, however, be confeſſed, that I met with great variety in the experiments of this kind made at different times. After that the bulk of the air ſhaken in water is increaſed to a certain degree, it then begins to decreaſe continually; and, in proportion to this decreaſe, the air becomes gradually leſs good. When the experiment is tried in cloſe veſſels, the diminution cannot be obſerved; but I ſhall reſerve, for another opportunity, to ſpeak of the laws and cauſes of thoſe diminutions and augmentations, and the differences obſerved between common and other kinds of air, and when the experiments are tried in water, &c.

For the preſent I ſhall only mention, that if the dephlogiſticated air be ſhaken in the tube, in the manner above mentioned, not only it does not increaſe its bulk, but it begins to diminiſh from the very beginning of the operation, and it continually loſes more and more of its bulk, and with its bulk of its purity.

This laſt mentioned property of the dephlogiſticated air ſeems to ſhew, that this is a fluid much different from common air, becauſe it has its peculiar properties by which it differs from common air not from more to leſs only, but entirely; as is ſhewn by the property this fluid has of being abſorbed by water; whereas common air receives an increaſe of bulk and elaſticity by being ſhaken in water.

All that I have been ſaying above, in order to give an idea of my method, and the words I uſe to expreſs the diminutions made by the mixture of nitrous air and other kinds of reſpirable air, is not ſufficient to obtain reſults conſtant and certain, ſo as to deduce any conſequences from them. Even after that all the elements are corrected, and all the cauſes of error hitherto unknown or neglected by the moſt diligent obſervers, are avoided (which cauſes ſenſibly alter this kind of experiments) it is abſolutely neceſſary to follow always a conſtant and equal method, not only in the act of introducing the various kinds of air into the tube, but alſo after the mixing of the two kinds of air. The leaſt variation of circumſtances cauſes very great variations in the reſults of the experiments, and theſe variations of circumſtances are ſo minute that I never ſaw any of the perſons that obſerved my experiments who could diſcover them, although apprized of my deſign. The neglect alone of this uniformity of operation may occaſion an error of from 20 to 50 parts and upwards in the experiment with common air; but with dephlogiſticated air the error is incomparably greater, ſo much that the ſame quality of air a moment after may decompoſe even a double quantity of nitrous air; ſo that the pureſt common air would appear to be noxious, and phlogiſticated air; and the dephlogiſticated air would appear leſs good, and even noxious: for, by the teſt of nitrous air, it might appear little different from a mixture of dephlogiſticated and phlogiſticated air.

I ſhall take another opportunity to ſpeak of all the particulars relative to my method; but for the preſent I muſt mention, that, after uſing all the cautions I am maſter of; the greateſt error in the diminutions is not greater than one ſixtieth of the common air introduced in the tube: ſo that, after having made five or ſix experiments ſucceſſively, the probable error is ſo ſmall that it may be ſafely neglected. But, if one choſe to operate upon a quantity of air nine or ten times greater than that I commonly uſe, the error could not be 1000th part of the quantity of the air, which quantity would not be more than a few cubic inches. In the deſcription of my method I ſhall alſo mention the means by which I obtain nitrous air of a nearly equal and conſtant goodneſs, and in what manner I can refer my experiments to ſome conſtant ſtandard. For want of this method it is that we are not certain of the obſervations made about the ſalubrity of common air in different places, and that no tables of its changes have been made.

I have not the leaſt heſitation in aſſerting, that the experiments made to aſcertain the ſalubrity of the atmoſpherical air in various places, in different countries and ſituations, mentioned by ſeveral authors, are not to be depended upon; becauſe the method they uſed was far from being exact, the elements or ingredients for the experiment were unknown and uncertain, and the reſults very different from one another.

When all the errors are corrected it will be found, that the difference between the air of one country and that of another, at different times, is much leſs than what is commonly believed, and that the great differences found by various obſervers are owing to the fallacious effects of uncertain methods. This I advance from experience: for when I was in the ſame error, I found very great differences between the reſults of the experiments of this nature which ought to have been ſimilar; which diverſities I attributed to myſelf rather than to the method I then uſed. At Paris I examined the air of different places at the ſame time, and eſpecially of thoſe ſituations where it was moſt probable to meet with infected air, becauſe thoſe places abounded with putrid ſubſtances and impure exhalations; but the differences I obſerved were very ſmall, and much leſs than what could have been ſuſpected, for they hardly arrived to one fiftieth of the air in the tube. Having taken the air of the hill called Mont Valerien at the height of about 500 feet above the level of Paris, and compared it with the air of Paris taken at the ſame time and treated alike; I found the former to be hardly one thirtieth better than the latter. In London I have obſerved almoſt the ſame. The air of Iſlington and that of London ſuffered an equal diminution by the mixture of nitrous air; yet the air of Iſlington is eſteemed to be much better. I have examined the air of London, taken at different heights; (for inſtance, in the ſtreet, at the ſecond floor, and at the top of the adjoining houſes) and have found it to be of the ſame quality. Having taken the air at the iron gallery of St. Paul's cupola at the height of 313 feet above the ground, and likewiſe the air of the ſtone gallery which is 202 feet below the other; and having compared theſe two quantities of air with that of the ſtreet adjoining; I found, that there was ſcarce any ſenſible difference between them, although taken at ſuch different heights.

In this experiment a circumſtance is to be conſidered, which muſt have contributed to render the above mentioned differences more ſenſible; this is, the agitation of the air of the cupola, for there was felt a pretty briſk wind upon it, which I obſerved to be ſtronger and ſtronger the higher I aſcended; whereas in the ſtreet, and indeed in all the ſtreets I paſſed through, there was no ſenſible wind to be felt. This experiment was made at four in the afternoon, the weather being clear. The quickſilver in the barometer at that time was 28,6 inches high, and FAHRENHEIT'S thermometer ſtood at 54°. After having related all theſe circumſtances, it will be neceſſary to give the mean reſult of all the various experiments made upon each of thoſe quantities of air treated after my method with the nitrous air. The air of the ſtreet gave II–13, II+6; the air of the ſtone gallery, which was 202 feet high, gave II–14, II+5; and the air of the iron gallery, which was 313 feet high, gave II–14, II+5. The reſults of the two laſt experiments are exactly the ſame; and that of the firſt is hardly at all different from them. Mr. CAVALLO, who has ſhewn the literary world his ability in examining nature, aſſiſted me in thoſe experiments, ſo that a miſtake can hardly be ſuſpected. From this we clearly ſee, how little the experiments hitherto publiſhed, about the differences of common air, are to be depended upon. In general I find, that the air changes from one time to another; ſo that the differences between them are far greater than thoſe of the airs of different countries, or different heights; for inſtance, I have found that the air of London, in the months of September, October, and November, 1778, when treated with the nitrous air, gave II–6, II+15, which is a mean reſult of many experiments which differed very little from each other. The 26th day of November laſt, I found the air for the firſt time much better, for it gave II–12, II+12; but the 14th of February laſt, the air gave II–18, II−18, II+7; from whence it appears, that the air of this 14th of February was better than it had been for ſix months before. There can be no doubt of the accuracy of the experiments, becauſe I compared the air taken at different times with that which I had firſt uſed in the month of September, and which I had preſerved in dry glaſs bottles accurately ſtopped. Now if the formulæ expreſſed above are compared together, it will be found, that the difference between the firſt terms is of twelve parts, and that between the latter of ſeven; that is, of one tenth and one twenty-fourth of the whole quantity of air: which are much greater differences than thoſe mentioned above. Notwithſtanding this, I could not perceive any particular change of health, or facility of breathing, ariſing from thoſe changes of the ſalubrity of the atmoſpherical air; and I am informed, that no particular diſeaſes appeared which could indicate any remarkable change of air.

Nature is not ſo partial as we commonly believe. She has not only given us an air almoſt equally good every where and at every time, but has allowed us a certain latitude or a power of living and being in health in qualities of air which differ to a certain degree. By this I do not mean to deny the exiſtence of certain kinds of noxious air in ſome particular places; but only ſay, that in general the air is good every where, and that the ſmall differences are not to be feared ſo much as ſome people would make us believe. Nor do I mean to ſpeak here of thoſe vapours and other bodies which are accidentally joined to the common air in particular places, but do not change its nature and intrinſical property. This ſtate of the air cannot be known by the teſt of nitrous air, and thoſe vapours are to be conſidered in the ſame manner as we ſhould conſider ſo many particles of arſenic ſwimming in the atmoſphere. In this caſe it is the arſenic, and not the degenerated air, that would kill the animals who ventured to breathe it.

In this place, therefore, I do not mean to ſpeak of thoſe changes which do not immediately alter the nature of the air itſelf. The other ſtates of that fluid are of another kind, and they are not to be examined by means of nitrous or inflammable air (the uſes of which laſt, I ſhall ſhew on another occaſion). The ſame thing may be ſaid of thoſe vapours or particles which may be good for reſpiration, and do not change the nature of the air. Some vegetables, for inſtance, can diffuſe through the air ſuch exhalations as may be of real uſe to the animal œconomy when they are breathed for a long time, or imbibed by the pores of the ſkin. I remember to have often put various flowers, as roſes, pinks, &c. in veſſels full of common air confined by quickſilver, where I left them for ſeveral hours; after which time I found, that the air was not at all altered, but that various animals ſeemed to breathe it very well, notwitſtanding that the flowers filled the greateſt part of the veſſels. On the contrary, I have found, that the vapours ariſing from lime ſlacked in water, either do not alter the air at all, or very little; though when breathed with the air they occaſion the death of animals.

I would not have any body ſuppoſe, that I think it of little importance to know the goodneſs of the atmoſpherical air, and the changes it undergoes. On the contrary, I believe it to be a very uſeful inquiry for mankind, becauſe we do not yet know how far one kind of air more than another may contribute to a perfect ſtate of health; nor at what time ſmall differences may become very conſiderable, when one continues to breathe the ſame kind of air for whole years, eſpecially in ſome kind of diſeaſes. An exact method of examining the goodneſs of common air may even be uſeful to poſterity, in order to aſcertain whether our atmoſphere degenerates in a length of time. This curious inquiry, together with the method, &c. are the production of this eighteenth century; and our deſcendants muſt have ſome gratitude for the philoſophers who found out, as well as for thoſe who improved it. If our anceſtors had known and tranſmitted it to us, we ſhould, perhaps, at preſent be able to judge of one of the greateſt changes of our globe, of a change which very nearly intereſts human life.